★ REVIEWS ★

Specters of Marx: A Review of *Adam's Tongue* by Derek Bickerton

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Derek Bickerton. 2009. *Adam's Tongue: How Humans Made Language, How Language Made Humans*. New York: Hill and Wang.

Die Sprache ist so alt wie das Bewußtsein—
die Sprache ist das praktische, auch für andre Menschen existierende,
also auch für mich selbst erst existierende wirkliche Bewußtsein,
und die Sprache entsteht, wie das Bewußtsein, erst aus dem Bedürfnis,
der Notdurft des Verkehrs mit andern Menschen.
Karl Marx & Friedrich Engels, *Die deutsche Ideologie* (1846)

1. Introduction

Jacques Derrida entitled one of his books *Spectres de Marx*. Pun intended. Derrida was playing with the plural 'specters' to suggest that many ghosts of Marx might be haunting modern Western society, while, at the same time, he was reminding us of the opening words of the *Communist Manifesto*: "*Ein Gespenst geht um in Europa*". Well, it seems that as far back as 1995 he was quite right after all, and that the specters of Marx materialize themselves in the most unexpected places: Derek Bickerton has just invoked one of them.

Derrida's point was that some of these materializations were good. Not just good, but necessary. Unfortunately, Bickerton's invocation in the context of language origins does not appear to be as necessary—nor as good, for that matter—as it is, according to Derrida, in other areas of contemporary Western thought.

Ein Gespenst geht um in Evolutionslinguistik—the specter of communication; but now in a Marxist disguise.

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Not that Bickerton appears to be aware of what his proposal actually boils down to. Quite to the contrary, he seems to be in absolute oblivion of this. He seems in fact to be pretty convinced that he is doing hardcore biology; but he is not. Indeed, if we were to limit ourselves to provide a short version of Bickerton's model, we would already have an off-the-shelf paragraph to offer, namely, Marx and Engels's quote above. But Bickerton needed a whole book to say more or less the same, so we'd rather pay attention to the details.

This will be our task for the rest of the review.

2. Constructing Niches in the Pleistocene Savanna

Bickerton has buried six feet down his original ideas about language evolution. *Language & Species* (Bickerton 1990) has ceased to be the basic reference for his position, this book has now been overthrown by *Adam's Tongue* (AT). So, no more Paradox of Continuity, no more 'language as a representational system', nothing. Now there's continuity and communication.

We are convinced that this will appall many readers. It is, after all, a rather radical change, indeed. As a matter of fact, we could articulate our review along this axis, and try to show that the continuist approach that Bickerton now embraces is hopeless, that no account of the origins of language will ever work as long as it is based on the idea that communication evolves. Because communication does not evolve, nor other functions for that matter.

But we are not going to do this. And the reason why we are not going to do it is this: Despite the fact that we have criticized these ideas elsewhere (Balari & Lorenzo 2009a, in press), we are coming to the conviction that this is becoming a sterile debate—that one either believes in the 'communicative approach' or one doesn't, that an intellectual wall has been built between two radically different ways of approaching the problem of the origins of language, and that very little can be done to permeate this barrier. We are still pretty convinced that the communicative approach is wrong and, therefore, our review of AT could easily dismissed as a matter of ideology, so we are not going to take this line of attack. Especially, because the problems with AT go well beyond this debate, and it is on these problems we want to focus our attention to.

So, what's *so* wrong with AT? We'll get to that in a moment, but first, a quick overview of the proposal. AT's main theme is that language could have evolved from *any* animal communication system should the appropriate conditions had been met. The crux of the matter is that such conditions are not internal to the organism, but, rather, external to it—they are intimately linked to the kind of ecological niche the organism in question entered (or constructed; more about this below) at some point during its life history. This is how the Paradox of Continuity is (dis)solved: Continuity between animal communication systems is possible, but it is not observed between primates and humans because extant primate species do not occupy the appropriate ecological niche, whereas the ancestors of humans did enter this niche. Language is the inevitable consequence of the kinds of selective pressures that certain niches impose over the communication systems of organisms. According to Bickerton such pressures are intimately connected to the kind of foraging strategy 'chosen' by the

organism, which, in the case of human's ancestors corresponds to what Bickerton calls 'high-end scavenging'.

The story goes more or les like this: After reviewing some circumstantial evidence, the author concludes that, at some point during the transition between Australopithecines and *Homo*, the old 'low-level scavenging' niche was abandoned and the construction of the new 'high-level scavenging' niche started. Low-level scavenging was the strategy our ancestors were forced to use in order to acquire food resources once a number of climatic changes turned a forest environment, rich in fruit, nuts, seeds and roots, into a dry savanna, where such resources are notably scarce. The only alternative was to profit from the carcasses of dead animals, but, since hyenas, vultures and other carnivores were tough competitors, our ancestors could only benefit from what these other animals left to them, namely bones. For a while, then, we fed on bone marrow, which we acquired thanks to very crude stone tools we used to break the thicker bones that carnivore teeth were unable to pierce. Suddenly, however, things change and there seems to be evidence that somehow our ancestors managed to outcompete other scavengers and were getting there first and keeping for them the best morsels of meat. How all this came to happen? The answer, of course, is that we invented language. Well, not really, just some kind of protolanguage, but already with a critical feature that was to pave the way to full-fledged language. The critical feature in question is displacement. Already in *Language & Species*, Bickerton (1990: 152–154) hinted at the idea that scavenging might have something to do with the origins of language, because it somehow seems to require recruiting the other members of the group to get the most of the food resource. In AT, he takes this idea to the extreme (there is some continuity in Bickerton's ideas after all) and he presents us with the following just-so story: Our ancestors *needed* language—protolanguage with displacement, that is—once they had started constructing the high-end scavenging niche. They needed it to tell the other members of the group about what they had found, that a nice deinotherium was lying there, dead stiff, over the hill, and thus, that they could all run there with their crude stone tools and throw them at their competitors, and with their cries freak them all out and frighten them all away in order to keep the whole carcass for themselves. So the story goes.

"But wait, *how* did language came to be?", you may be tempted to ask. Easy, Bickerton answers, it's just a matter of niche-construction. Look at ants, look at bees, they are also foragers who recruit. And they have displacement. It's just a matter of getting into the appropriate niche and it will generate the appropriate needs to start the appropriate feedback process eventually leading to language. It's just inevitable.

"But, sorry, wait again", you would probably object, "ants and bees *may* have displacement, but they *don't* have language, do they?". So there must be some other important difference between them and hominids. Big brains, perhaps? Not necessarily so, Bickerton retorts. Actually, it's the other way round, brains got bigger because of language. It's a fallacy to believe that one needs a big brain to get language.

So, the conclusion is clear: "Other animals didn't get language because, bottom line, they didn't need language." (AT: 24).

Now we have all the necessary elements of Bickerton's model in place, but, for the reasons exposed above, we would like to concentrate on just two of them. First, by appealing to the 'Big Brain Fallacy', Bickerton denies any role to proximate/internal causes in the origins of language, leaving all the work to ultimate/external causes, namely behavior. Second, the evolutionary mechanism responsible of the emergence of language was a process of niche-construction in the context of high-end scavenging which—necessarily—eventually yielded a communication system with the property of displacement.

In the two remaining sections of this review we would like to show that the first assumption is a vivid example of the most cavalier oversight of current knowledge concerning brain evolution ever; as for the second point, we will argue that Bickerton misinterprets for his own benefit Niche-Construction Theory (NCT; Odling-Smee *et al*. 2003) to turn it into a naive environmentalism of sorts, going well beyond the environmentalism exemplified by Marx and Engels, or, for that matter, Rousseau.

3. The Big Brain Fallacy, or Why Ants and Bees Could Have Language (If They Don't Have It Already)

If you want to pass the buck to the environment in accounting for some evolutionary process, you'd better get rid of all possible proximate causes that might interfere in your goals. That's what the Big Brain Fallacy is for.

We'll never be able to express Bickerton's position better than him, so we'd rather quote him in full:

> If, as some have claimed, language was an invention of folk with big brains, it would be doubly unique. In addition to being the only system of its kind in nature, it would be the only biologically based behavior that had ever been consciously and deliberately created. And if you believe we can deliberately create biologically based behaviors, I have a couple of bridges you might contemplate purchasing.
>
> But the real clincher is this. Brains don't grow by themselves, of their own volition; they grow because animals need more brain cells and connections to more effectively carry out any new things they are beginning to do. In other words, brain-size increase doesn't drive innovation—innovation drives brain-size increase. (AT: 34)

Sic. No comment about the first paragraph. It speaks for itself. As for the second, we could start by saying that, of course, brains don't grow by themselves, of their own volition, but who *ever* said anything like that? (No references given concerning that particular point in AT.) This would really be a rather outrageous view of how organisms and organic structures develop and evolve. We're still wondering where Bickerton picked up this idea. Certainly not from Aristotle, nor from the preformationists, nor from the recapitulationists either; certainly not from any contemporary work on biology. Looks like a straw man.

The remaining lines of the second paragraph deserve some comment, however. They are important because they concern the directionality of the arrow of causality in developmental/evolutionary processes. As it is clear from the quote above, Bickerton sees a single causal arrow pointing in one direction only,

namely from the outside to the inside. In order to fully appreciate the kind of naive biological thought underlying this contention, we need to get into a detailed analysis of Bickerton's interpretation of niche-construction, a task that we undertake in the next section, but let us point out for the time being that such a view where all the power goes to the environment runs counter all current conceptions of how evolutionary processes work; it's not even Darwinian. It is not Darwinian, because, among other reasons, by attributing such an immense power to the environment, it is neglecting the effect of any existing structural constraint on organisms that might prevent them to evolve in some specific directions. For Bickerton, given some appropriate genetic fiddling, the appropriate environmental conditions could turn ants and bees (they have displacement, don't they?) into full-fledged linguistic beings (if they aren't already; see AT: 136–137).

Turning our attention back to brains, we would be much more cautious before making such a categorical statement concerning brain evolution. As pointed out by Striedter (2005: 134), "we are still far from consensus when it comes to the biological significance of evolutionary changes in overall brain size", so we do not know exactly why brains got bigger. What we *do* know, however, is that brains, since they were invented, have been getting steadily bigger and bigger (at least in vertebrates; Finlay & Darlington 1995, Striedter 2005); and we also know that some of these episodes of brain growth correlate with behavioral innovations (Striedter 2005), which is hardly surprising given the fact that all behavior supervenes on brain structure and organization. Granted, but from this it does not follow that novel behavior actually *causes* brain-growth. In fact, there seem to be reasons to expect that the opposite is true. These reasons are mostly developmental and concern the fact that developmental programs are extremely conservative (Shubin *et al*. 1997, 2009), and the case of brains is no exception (Finlay & Darlington 1995), so it is now well accepted that a very important source of novelty are some specific small perturbations of developmental programs that often give rise to some kind of heterochrony (Gould 1977, Parker & McKinney 1999). Again, this is probably not a knock-down argument against Bickerton's position, but it becomes one when coupled with the following quote:

> As for 'the rewiring of the brain,' brains don't rewire themselves for no reason, or because they just feel like a spot of rewiring. Brains rewire themselves, to the extent they do, because things are happening in the outside world, things that give individuals with rewired brains an advantage over individuals without them. (AT: 183)

Well, *this* is plainly false. Brains do rewire themselves when they get bigger: "[B]rains, like companies, must reorganize as they increase in size" (Striedter 2005: 127). And for very good reasons. As pointed out by Hofman (2001), as a brain grows bigger, soon a problem of connectivity arises given the fact that cortical and subcortical structures do not grow at the same rate (Finlay & Darlington 1995, Kaskan & Finlay 2001, Striedter 2005), which is linear for the latter but exponential for the former. As a consequence of that, and to ensure efficient neuronal communication, brains tend to compartmentalize and to develop laminae with groups of highly interconnected neurons (Deacon 2000), the net result being

that bigger automatically means rewired. Thus, not only do brains rewire themselves, but when they do it, it is because something is happening in the *inside* world, not outside.

We can now see how the 'Big Brain Fallacy' collapses: Even if brain growth could eventually be attributed to some behavioral change (which is dubious), such increase in size would have necessarily implied a general rewiring and reorganization of the system in order to preserve its efficiency, with unpredictable consequences for the cognitive capacities of the organism and, no doubt, with an enormous potential for introducing behavioral novelty (Balari & Lorenzo 2008, 2009b). Thus, the more parsimonious (and logical) hypothesis given current evidence is that brain growth drives innovation, not that innovation drives brain-size.

4. How Bickerton Invented Niche-Construction

Now it's time to be fair. Fair with NCT, which cannot be held responsible of the fact that the evolutionary model adhered to by Bickerton in AT receives the same name.

Let us first try to disentangle the different threads of the fabric spun by the author in order to find the keys of the evolutionary mechanisms that he seems to have in mind. We'll start with a couple of quotes:

> But behavior and environment aren't watertight compartments—they're intimately linked; they shape each other into a lock-and-key fit. And fit is precisely what is meant by 'adaptation'. (AT: 64)

> So a feedback process begins, a two-way street in which the animal is developing the niche and the niche is developing the animal, until you get the lock-and-key fit between animal and niche that makes people say, "But there must be a designer!" (AT: 99–100)

At first sight, this looks like garden-variety adaptationism. We even find the very same metaphors Richard Lewontin once used to define it:

> The concept of adaptation implies that there is a preexistent form, problem, or ideal to which organisms are fitted by a dynamical process. The process is adaptation and the end result is the state of being adapted. Thus a key may be adapted to fit a lock by cutting and filing it. (Lewontin 1985a: 67)

Note that, despite of the fact that Bickerton appears to assume that organisms posses some kind of causal power over environments, his characterization of the process is virtually identical to that of Lewontin's: The niche is there, the organism enters it, and a process begins which will eventually culminate when the perfect fit is attained, at which point it will stop ("until you get the lock-and-key fit"). That niches are there, waiting, until some organism decides it's about time to move and start making a new living in it follows from a number of statements we find in AT, like this one, for example:

> The size niche exists, permanently, within any order, simply because if you're bigger than anything else around, you're virtually invulnerable to attack. (AT: 122)

So far, then, we seem to be within the bounds of a Darwinism of the most orthodox kind, as the model doesn't look really different from those of classical sociobiology (Wilson 1975, Dawkins 1982), where genes—and only genes—are the one and only mechanism of variation, and where natural selection, that is, the selective pressures generated by the different environments (or niches), acts as the only causal power, capable of modifying allelic distributions within populations and favoring only those alleles correlating with those behaviors which happen to be apt for the niche in question. But, although Bickerton recognizes that the Extended Phenotype model of Dawkins (1982) comes close to what he has in mind with his niche-construction, he explicitly rejects it (AT: 99) to plunge headfirst into something entirely different.

Let us go back to the classical model for a while: Genes produce random variation and natural selection favors one or another variant depending on the conditions of the environment. That is, the internal mechanisms of variation (genes) express themselves into phenotypes, which are visible (external) for natural selection. Thus, causality follows an outside-inside direction, but note that a basic precondition for this kind of causality to work is that some variation pre-exists that is internal in nature: Classical Darwinism never paid too much attention to developmental processes, because it always assumed that the gene-phenotype relation is simple or negligible; but what Darwinism never denied is that variation has its origins in *internal* mechanisms. What is Bickerton's view on this particular issue? Two more quotes from AT:

> Most often than not, behavior changes first, then the genes change to keep pace with it. (AT: 96)

> [O]nly external events can shape internal events, because only external events are visible to natural selection. (AT: 185)

At first sight, this is not too different from the classical model: The arrow of causality travels from the outside to the inside. Note, however, a critical difference concerning the nature of variation (and, maybe, also inheritance), since Bickerton is explicitly inverting the directionality of the phenotype-gene relation, which explains that we find him making such preposterous claims as the following:

> It was, after all, the development of powered flight that eventually caused genes normally devoted to building front legs to express themselves, among birds and bats, in the form of wings. (AT: 131)

So, first came flight and only thereafter genes started making wings. Note how Bickerton is here inserting a new causal level between the organism and the environment, a behavioral/functional level with an enormous creative power which, in fact, relegates genes (and any other proximate cause) to the role of mere puppets, ready to cater for whatever needs the organism may have: If we entered the flying niche, the need of flying would be created in us and we would start flying; later, genes will make wings for us: Like good old Arthur Dent in *The Hitchhicker's Guide to the Galaxy*, you just have to fail to hit the ground.

Beware, this is not to be confused with other models where an explicit

attempt has been made to integrate behavior into the causal chain of developmental processes in order to overcome the classical view of development as a simple, linear, gene-phenotype relation, and to see it instead as a complex process with multiple causal forces (e.g., Oyama 2000, Jablonka & Lamb 2005). Not at all. In AT, Bickerton explicitly despises the relevance of development in evolution (AT: 130): For him, development is in charge of building structures, parts of the body, not behaviors and, as he has explicitly stated several times in AT, evolutionary changes are driven by behaviors, so it's behaviors who determine what developmental processes have to do, not the other way round. Nothing in AT suggests the possibility of integrating everything that concerns development in a new notion of environment with behavior being one of these elements. As we will see presently, however, this integrative perspective *is* a constitutive idea of the notion of niche-construction.

This is not NCT; it is not classical Darwinism either, neither of the orthodox nor of the heterodox kind; it is, if anything, Lamarckism, but a crude and clumsy Lamarckism of sorts—typical of the most uninformed interpretations of the notion of the inheritance of the acquired characters; with, perhaps, a pinch of Rousseau. It is, in any case, a serious perversion of the ideas and theoretical underpinnings of NCT. Let's see why.

Since its inception, NCT has presented itself as a spin-off of the original proposals by Lewontin (1983, 1985b, 2000) to dispose of the adaptationist lock-and-key metaphor in favor of a constructive, dialectical model in which genes, organism and environment are an integral part of the same cyclic developmental process, where organism and environment co-construct each other in a never ending process (unless the organism goes extinct, of course). It is, in other words, an attempt to dissolve the traditional dichotomy between external and internal causes, and to grant the same role in the evolutionary process to all intervening factors, including, of course, natural selection (Laland *et al*. 2001a, Day *et al*. 2003), and thus recognizing the power of the organism to modify the selective pressures acting over it. Obviously, as it has been pointed out several times by the proponents of NCT (Laland *et al*. 1996, 2000, 2001b, 2003, and Laland & Brown 2006), these factors often pertain to the realm of what traditionally is seen as external, to the sphere of behavior or culture, but they never attributed to these factors the qualities Bickerton attributes to them in his own model.

"We [don't] have to struggle with the high-powered math that Odling-Smee and his colleagues use to justify their ways to population geneticists", Bickerton advises us (AT: 103). But if we do not follow his advice and take the trouble of examining their math, we will immediately discover that their models are a variety of epistatic selection population models with multiple *loci* (Font-devila & Moya 1999: chap. 6), where a third element R is added representing the environmental factor that is affected by the presence/absence of the different linked alleles, which, in turn, may affect the frequency in the population of one or another haplotype (e.g., Laland *et al*. 1996). That is, and following the two-locus model of Laland *et al*. (1996), we have two *loci* A and E, with alleles A/a and E/e, respectively, yielding the haplotypes AE, Ae, aE, and ae, such that variation in E determines changes that may occur in R, and variation in A determines the organism's fitness with respect to the environment modified by the phenotypic

traits defined by E. Thus, a feedback model is defined where variations in E cause changes in R which, in turn, favor one or another allele of A, dynamically bringing about changes in the frequencies of the different haplotypes in the population. We don't need to go into the fine mathematical details to see that this model has little, if anything, to do with the Bickertonian idea that "behavior changes first, then genes change to keep pace with it". Quite to the contrary, for the process to be able to start, a necessary precondition is the existence of some kind of genetic variation in the population. In a later paper (Laland *et al*. 2001b), the model was extended to the case where one of the two *loci* is not a genetic character, but some specific cultural activity, corresponding, for example to the oft-cited phenomenon of the expansion of the gene conferring greater adult lactose tolerance among certain human populations in association with persistent domestication of cattle and dairying activities (Durham 1991, Richerson & Boyd 2005). Clearly, in both models, as a consequence of some behavioral novelty, either with a genetic or with a cultural basis, the only genetic changes we observe concern the frequency of some previously existing allele in the population. What we certainly do not see is the kinds of changes Bickerton suggests in AT, where genes, with their plasticity and flexibility seem to be able to sanction any possible and imaginable behavior invented by the organism to satisfy its needs in the niche it currently occupies.

NCT is yet another attempt to integrate certain environmental and cultural factors in a more global and complete picture of evolutionary processes, with the aim of showing that the classical models represent a too simplistic view of evolution. It is therefore not surprising that several people had started thinking about a new evolutionary synthesis in biology, where the different alternative views are integrated that share the construction metaphor in their view of evolution, such as ecosystems ecology (Jones *et al*. 1994, 1997, and Wright & Jones 2006), evolutionary developmental biology (Hall 1999, Minelli 2009), phenotypic/developmental plasticity models (West-Eberhard 2003), ecological developmental biology (Gilbert 2001, Gilbert & Epel 2009) and, of course, NCT. In fact, some bridges have already been built between NCT and evo-devo, for example (Laland *et al*. 2008), and the shape that is starting to take form is the antithesis of the poor and uncouth biology that Bickerton is proposing. That's the difference between chatting about biology and actually doing some.

5. Conclusion

As we advanced at the beginning of this review, our criticisms do not focus on AT's communicative and continuist approach. We decided to skip that. It's just one of the many secondary factors that make AT a bad book.

Indeed, we could've added that its style is arrogant, full of irrelevant personal anecdotes, florid just-so stories about our ancestors scavenging in the savanna, and the odd bad joke dropped here and there just for the fun of it; plus some distasteful ad hominem argument to top it all. Or we could've said that AT doesn't actually explain how language evolved. Some kind of protolanguage with displacement, maybe, but language, with such things as agreement, questions, anaphora, in a nutshell with all those features that make the structure

of natural languages so complex, not really. Just skip everything and go to the last chapter and watch Bickerton's hand waving.

But these are just ancillary matters, secondary issues on the basis of which one is not supposed to judge scientific writing. So we centered our attention to scientific questions, and it is our hope to have shown that AT is not just bad evolutionary linguistics, it's bad science, very bad science. If it is science at all. It's something not even able to match the pre-Darwinian environmentalism of Marx and Engels.

References

Balari, Sergio & Guillermo Lorenzo. 2008. Pere Alberch's developmental morphospaces and the evolution of cognition. *Biological Theory* 3, 297–304.

Balari, Sergio & Guillermo Lorenzo. 2009a. Comunicación: Donde la lingüística evolutiva se equivocó. Report de recerca CGT-09-10, Centre de Lingüística Teorica, Universitat Autònoma de Barcelona.

Balari, Sergio & Guillermo Lorenzo. 2009b. Computational phenotypes: Where the theory of computation meets evo-devo. *Biolinguistics* 3, 2–60.

Balari, Sergio & Guillermo Lorenzo. In press. ¿Para qué sirve un ballestrinque? Reflexiones sobre el funcionamiento de artefactos y organismos en un mundo sin funciones. *Teorema: Revista Internacional de Filosofía*.

Bickerton, Derek. 1990. *Language & Species*. Chicago: University of Chicago Press.

Dawkins, Richard. 1982. *The Extended Phenotype*. Oxford: Oxford University Press.

Day, Rachel L., Kevin N. Laland & John Odling-Smee. 2003. Rethinking adaptation. The niche-construction perspective. *Perspectives in Biology and Medicine* 46, 80–95.

Deacon, Terrence W. 2000. Heterochrony in brain evolution. Cellular versus morphological analyses. In Sue Taylor Parker, Jonas Langer & Michael L. McKinney (eds.), *Biology, Brains, and Behavior: The Evolution of Human Development*, 41–88. Santa Fe, NM & Oxford: School of American Research Press/James Currey.

Durham, William H. 1991. *Coevolution: Genes, Culture, and Human Diversity*. Stanford, CA: Stanford University Press.

Finlay, Barbara L. & Richard B. Darlington. 1995. Linked regularities in the development and evolution of mammalian Brains. *Science* 268, 1578–1584.

Fontdevila, Antoni & Andrés Moya. 1999. *Introducción a la genética de poblaciones*. Madrid: Síntesis.

Gilbert, Scott F. 2001. Ecological developmental biology: Developmental biology meets the real world. *Developmental Biology* 233, 1–12.

Gilbert, Scott F. & David Epel. 2009. *Ecological Developmental Biology. Integrating Epigenetics, Medicine, and Evolution*. Sunderland, MA: Sinauer.

Gould, Stephen J. 1977. *Ontogeny and Phylogeny*. Cambridge, MA: The Belknap Press.

Hall, Brian K. 1999. *Evolutionary Developmental Biology*, 2nd edn. Dordrecht: Kluwer.

Hofman, Michel A. 2001. Brain evolution in hominids: Are we at the end of the

road? In Dean Falk & Kathleen R. Gibson (eds.), *Evolutionary Anatomy of the Primate Cerebral Cortex*, 113–127. Cambridge: Cambridge University Press.

Jablonka, Eva & Marion J. Lamb. 2005. *Evolution in Four Dimensions. Genetic, Epigenetic, Behavioral, and Symbolic Variation in the History of Life*. Cambridge, MA: MIT Press.

Jones, Clive G., John H. Lawton & Moshe Shachak. 1994. Organisms as ecosystems engineers. *Oikos* 69, 373–386.

Jones, Clive G., John H. Lawton & Moshe Shachak. 1997. Positive and negative effects of organisms as physical ecosystem engineers. *Ecology* 78, 1946–1957.

Kaskan, Peter M. & Barbara L. Finlay. 2001. Encephalization and its developmental structure: How many ways can a brain get big? In Dean Falk & Kathleen R. Gibson (eds.), *Evolutionary Anatomy of the Primate Cerebral Cortex*, 14–19. Cambridge: Cambridge University Press.

Laland, Kevin N. & Gillian R. Brown. 2006. Niche construction, human behavior, and the Adaptive-lag Hypothesis. *Evolutionary Anthopology* 15, 95–104.

Laland, Kevin N., John Odling-Smee & Marcus W. Feldman. 1996. The evolutionary consequences of niche construction: A theoretical investigation using two-locus theory. *Journal of Evolutionary Biology* 9, 293–316.

Laland, Kevin N., John Odling-Smee & Marcus W. Feldman. 2000. Niche construction, biological evolution, and cultural change. *Behavioral and Brain Sciences* 23, 131–175.

Laland, Kevin N., John Odling-Smee & Marcus W. Feldman. 2001a. Niche construction, ecological inheritance, and cycles of contingency in evolution. In Susan Oyama, Paul E. Griffiths & Russell S. Gray (eds.), *Cycles of Contingency: Developmental Systems and Evolution*, 117–126. Cambridge, MA: MIT Press.

Laland, Kevin N., John Odling-Smee & Marcus W. Feldman. 2001b. Cultural niche construction and human evolution. *Journal of Evolutionary Biology* 14, 22–33.

Laland, Kevin N., John Odling-Smee & Scott F. Gilbert. 2008. EvoDevo and niche construction: Building bridges. *Journal of Experimental Zoology (Mol. Dev. Evol.)* 310B, 549–566.

Lewontin, Richard C. 1983. Gene, organism and environment. In D.S. Bendall (ed.), *Evolution: From Molecules to Men*, 273–285. Cambridge: Cambridge University Press.

Lewontin, Richard C. 1985a. Adaptation. In Richard C. Lewontin & Richard Levins, *The Dialectical Biologist*, 65–84. Cambridge, MA: Harvard University Press.

Lewontin, Richard C. 1985b. The organism as the subject and object of evolution. In Richard C. Lewontin & Richard Levins, *The Dialectical Biologist*, 85–106. Cambridge, MA: Harvard University Press.

Lewontin, Richard C. 2000. *The Triple Helix: Gene, Organism and Environment*. Cambridge, MA: Harvard University Press.

Minelli, Alessandro. 2009. *Forms of Becoming: The Evolutionary Biology of Development*. Princeton, NJ: Princeton University Press.

Parker, Sue Taylor & Michael L. McKinney. 1999. *Origins of Intelligence: The Evolution of Cognitive Development in Monkeys, Apes, and Humans*. Baltimore,

MD: The Johns Hopkins University Press.
Odling-Smee, John, Kevin N. Laland & Marcus W. Feldman. 2003. *Niche Construction: The Neglected Process in Evolution*. Princeton, NJ: Princeton University Press.
Oyama, Susan. 2000. *The Ontogeny of Information*, 2nd edn. Durham, NC: Duke University Press.
Richerson, Peter J. & Robert Boyd. 2005. *Not by Genes Alone: How Culture Transformed Human Evolution*. Chicago: University of Chicago Press.
Shubin, Neil, Cliff Tabin & Sean Carroll. 1997. Fossils, genes and the evolution of animal limbs. *Nature* 388, 639–648.
Shubin, Neil, Cliff Tabin & Sean Carroll. 2009. Deep homology and the origins of evolutionary novelty. *Nature* 457, 818–823.
Striedter, Georg F. 2005. *Principles of Brain Evolution*. Sunderland, MA: Sinauer.
West-Eberhard, Mary Jane. 2003. *Developmental Plasticiy and Evolution*. New York: Oxford University Press.
Wilson, Edward O. 1975. *Sociobiology: The New Synthesis*. Cambridge, MA: Belknap Press.
Wright, Justin P. & Clive G. Jones. 2006. The concept of organisms as ecosystems engineers ten years on: Progress, limitations, and challenges. *BioScience* 56, 203–209.

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